LETTERS TO THE EDITOR.

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The Disaster to Submarine A1.

AT the inquest on the victims of the disaster to submarine A1, Commander Bacon is reported to have expressed the opinion that as the result of the collision every soul on board was instantly stunned, since the failure to set in action the mechanism for bringing the boat to the surface could not otherwise be accounted for. It is surprising that this opinion should have been received and adopted without comment by both the coroner and the lay Press, seeing that such a result is contrary to all experience of collisions at sea. The occupant of the conning tower, which was the part struck, was no doubt stunned, probably killed, by the blow, but it is difficult to believe that the same fate should have befallen every other person on board, however remote from the point of concussion.

The fact that the naval authorities can suggest no other reason for the failure to rise to the surface after the collision is not in itself a sufficient justification for the acceptance of an opinion which, from the physiological point of view, is, to say the least, highly improbable, and certainly requires confirmation by experiment.

University, Edinburgh, May 1. E. A. SCHÄFER.

The Life-history of Radium.

EVIDENCE of a convincing nature is rapidly accumulating to the effect that helium may be produced as a result of the disintegration of the radium atom. On the other hand, it has been suggested by Rutherford and others that radium is analogous to the first products of the disintegration of uranium and thorium—to the substances known as uranium X and thorium X—rather than to those elements themselves. Such an idea points to a search for the parent atom, by the dissolution of which radium is formed.

In Prof. Rutherford's recent book on radio-activity, reasons are given for suspecting that in uranium itself we shall find the origin of radium. The atomic weight of uranium is greater than that of radium. Radium is discovered in minerals rich in uranium, and the amount of radium in good pitchblende is about that to be expected on the view that a balance exists between the rate of development of the radium by the uranium present and the rate at which it decays by the ordinary process of radio-activity.

My wife and I have been investigating lately the slight amounts of radium emanation that are almost invariably found in samples of salts and oxides of uranium sold as chemically pure. By the kindness of Mr. H. J. H. Fenton we have been able to examine several specimens of uranium compounds, known to have been preserved in the Cambridge University Chemical Laboratory for periods of from seventeen to twenty-five years. In all cases greater amounts of radium emanation have been obtained from these old specimens than from more recently prepared samples of the corresponding compounds.

It is, of course, possible that a limited number of such results may be accidental, and, in order that indirect evidence of this kind should possess any weight, enough specimens must be examined to enable us to deal with the subject statistically. I should be very grateful if anyone possessing uranium compounds of known pedigree, prepared thirty years ago or upwards, would either test them quantitatively for radium emanation, or send a few grammes of them to me for examination.

If, in most cases, an excess of radium is discovered in the older samples, it would be presumptive evidence in favour of the view that radium is formed by the disintegration of uranium, but the possibility of some general change in the methods of preparation of uranium salts renders even such a confirmation of doubtful validity.

The only convincing evidence would be supplied by tracing the gradual growth of radium in a mass of a compound of uranium. At first sight, it would seem that the time required for such growth would put the possibility of such a confirmation beyond the reach of one human life. But a short calculation shows that the attempt is not so hopeless as might be imagined.

The average life of a radium atom is taken by Rutherford, on a minimum estimate, as about fifteen hundred years. The process of decay occurs in a geometrical progression, and thus in one year about half a milligramme per gramme of radium should disintegrate. On a maximum estimate for the life, the fraction disintegrated per year is 1/100 milligramme. Taking this maximum estimate as the least favourable for our purpose, we see that in one year the one hundred thousandth part will break up.

If in pitchblende, radium is in radio-active equilibrium with its source of supply, the same fraction must be replaced in the year by the disintegration of uranium. In presence of a large excess of uranium, the production of radium would go on at a constant rate. Thus in one year about the one hundred thousandth part of the proportion of radium in pitchblende would be developed in an equivalent mass of uranium.

We find that, using a good electroscope, it is easy to detect with certainty the radio-activity from the radium emanation evolved on heating a milligramme of good pitchblende. In order to produce from uranium an amount of radium large enough to detect by its radio-activity in a reasonable time—let us say one year—it is merely necessary to work with a sufficient quantity of uranium to give, in that time, a mass of radium of which the emanation has an activity equal to that evolved from a milligramme of pitchblende. The requisite quantity of uranium is clearly about $0{\cdot}001 \times 100000 = 100$ grammes. This, as we said, is a maximum estimate; it is probable that less would suffice.

In this manner, by putting on one side a few hundred grammes of some compound of uranium, carefully freed from radium and tested for emanation, it should be possible to detect the growth of radium in a time measured in months, or, on the other hand, to show that it is necessary to look elsewhere for the parent atom of radium.

At the present time we have such an investigation in progress, and trust that eventually we may obtain definite results. But, in the hope that others may undertake a similar task, I venture to place the principles of the method before your readers. On such a fundamental point, several independent experiments are greatly to be desired.

W. C. D. WHETHAM.

Upwater Lodge, Cambridge, April 30.

Graphic Methods in an Educational Course on Mechanics.

THOUGH no one, I venture to think, will gainsay Mr. W. Larden's main contention that "analytical methods give a grasp of the principles of statics, while graphical methods disguise them," yet it should not be forgotten that the analytical treatment has its own set of snares and pitfalls.

Mechanics is a physical science, and like other sciences should be approached from the experimental side. If the initial stages are treated experimentally, the principles underlying the subject will come prominently into view. One need only mention the principle of moments, which every boy has surely grasped, in a general sort of way, long before he has opened a text-book on statics. He has only to carry out a few simple experiments on levers to find out the law for himself in its exact form. Let the beginner hang up two spring balances from nails and then attach a weight by a couple of strings to the hooks of the balances, and he will soon discover for himself whether or not the pulls in the strings are proportional to their lengths.

The graphical treatment lays stress on the empirical and tentative side, which in the symbolical is completely lost sight of. But the superlative advantage of graphical work is its essentially practical character. All cases of a problem can be solved with equal facility. Ladders are not as a rule inclined to the ground at an angle of 60°, coefficients of friction are never quadratic surds, and weights of $\sqrt{2}$ poundals belong to some other world which is not the one in which we live. Again, the question is on a screw jack, and a boy taking $\pi = 22/7$ has worked out an answer

to four or five significant figures, and in consequence expects to get greater credit than his more indolent neighbour who has been content with two or three significant figures. Instances might be multiplied; they constitute the daily purgatory of every teacher. Something surely is to be said for a method which avoids these absurdities.

Analytical methods have so dominated the elementary text-book that many boys have the idea that statics is practically useless. They have no notion, for instance, that graphic statics lies at the foundation of bridge construction. Besides, in how many questions in the elementary text-book is the principle involved wholly obscured, because a trigonometrical conundrum is required and not an application of the conditions of equilibrium to give the unknown forces? In a popular text-book one-third of the questions at the end of one of the chapters are of this character. Is it to be wondered at that the average boy gets the idea that mechanics is a subtle epilogue to trigonometry?

Each question treated graphically should be regarded in the light of an experiment, in which the student should get the best result available with the means at his disposal. In any actual problem the *data* themselves are not correctly known, and the *quaesita* are therefore subject to all sorts of cumulative errors. This he quickly finds out by comparing his result with that of his neighbour, and he readily gets a notion of the degree of accuracy that he himself with pencil and ruler is capable of.

Mr. Larden writes:—"a student well trained in analytical methods can always pick up graphical methods rapidly when he needs them for special work." But will he do so? The engineer is not trained in analysis and allowed to adopt a graphical method when a specific problem arises. My experience is that the student, who has mastered analytical methods, is apt to consider graphical work as drudgery, and when called upon to solve a question graphically does not treat it with sufficient respect, and gets an indifferent result. A certain amount of finesse and judgment in choice of scale and of position of the initial force or load is required "to fit the diagram on to a given sheet of paper." This can be acquired only by practice.

Unfortunately it is too true that "graphical work consumes an amount of time that seems out of proportion to the mental training and knowledge of principles gained," but only when applied to too many similar questions. This, however, is misusing, not using the method.

I believe the best results will be obtained when the two methods are used side by side. They are strictly complementary, and the merits of each supply the deficiencies of the other. R. M. MILNE.

R.M. Academy, Woolwich.

Asser and the Solar Eclipse of October 29, 878.

UNDER the date DCCCLXXIX, Asser, in his "Life of King Alfred," gives the following entry:—"*Eodem anno eclipsis solis inter nonam et vesperam, sed proprius ad nonam, facta est.*" The oldest manuscript of the Anglo-Saxon Chronicle also notes an eclipse in 879, but it cannot be doubted that in each case the reference is to the eclipse of October 29, 878, which was total in South Wales and southern England. Particulars of the eclipse are given by Mr. Maguire in the *Notices* of the Astronomical Society, vols. xlv., 400, and xlvi., 26. The sun rose totally eclipsed in 73° N. and 42° 8′ W. at about 9.53 local time, and the central line of the eclipse, after passing near Dublin, Aberystwith, Dover and Fulda, went off the earth at sunset about 130 miles south of Moscow at 4.20 local time; St. David's, Winchester and London were within the limits of totality. With regard to the hour of the eclipse, it is needful to consider not only mean time and apparent time, but also natural time, which was the kind of time then in use, according to which the period between sunrise and sunset was conceived to be divided into twelve hours, which were, of course, much shorter in winter than in summer. As the sun rose at London on the day of the eclipse about 7.20, the natural hour would have contained only about 47 minutes of mean time. Mr. Maguire gives the middle of the eclipse at St. David's about 1.12, and at London about 1.18 mean time, and subtracting the equation of time, about 15 minutes, we have 12.57 and 1.3 for the apparent time as shown by a sundial; correcting for natural time, we obtain 1.13 for St. David's and 1.20 for London. Finally, making allowance for the difference of longitude, we see that totality occurred at St. David's at 12.46, and at London at 1.20, according to local time as shown by a waterclock, or some other time-keeper, properly regulated to mark the natural hours. We now have to consider what Asser meant by *Nonam* and *Vesperam*. Those who have written about the passage have taken *Nonam* to be identical with *Nonam Horam*, but probably they have not been right in doing so. It is shown in the "Dictionary of Christian Antiquities" (i. 793) that the day and night were divided into four equal parts, and that each quarter of the day was named after the last hour in it. "None embraces the seventh, eighth and ninth hours; and the last called Duodecima contains the tenth, eleventh and twelfth, ending at Sunset." Asser, however, evidently uses *Vespera* for *Duodecima*. *Nona* is, in fact, noon, the point when the sun is on the meridian, the beginning of the seventh hour, and *Vespera* is the point half-way between noon and sunset, in this case 2.20 mean time and 3.0 natural time. Thus what Asser says is this, that the eclipse was total at a point of time between noon and 1.30 natural time, and we see that the statement is true for any point in England or Wales. If we could be sure that the sentence about the hour of the eclipse was written by Asser of St. David's, it would be a very strong argument, indeed, for the genuineness of the book which is called by his name, for it fixes the moment of the eclipse correctly to within seventy minutes of mean time for any place at which it is possible that the book could have been written. C. S. TAYLOR.

Banwell Vicarage, April 23.

"Abdominal Ribs" in Lacertilia.

IT is usually stated in text-books that among living reptiles only the Crocodilia and Hatteria are furnished with abdominal ribs or parasternum: that is, of course, in the condition of thin pieces of bone lying between the ventral muscles and underlying the true ribs, for no one doubts that the plastron of the Chelonia is the same structure exaggerated. There has been some little confusion between the abdominal ribs and the ventral moieties of the true ribs in Lacertilia, which is cleared up by Dr. Gadow in his contribution to the "Cambridge Natural History." Dr. Gadow correctly observes of the geckos that they possess very long and slender post-thoracic ribs, "which meet each other in the middle line, in this case bearing an extraordinary resemblance to the so-called 'abdominal ribs' of other Reptiles." The statements as to "abdominal ribs" made by M. Boulenger in his catalogue of the lizards in the British Museum appear to me to refer to true ribs. Of the Scincidæ, he remarks that ossified abdominal ribs are absent." Curiously enough, it is precisely in this group that I find a parasternum. In *Tiliqua scincoides* the ventral musculature is divided by the usual tendinous septa into successive "myotomes," the tendinous intervals being distinctly ossified; there are several pairs of these bonelets which seem to be exactly like those of Hatteria, with which I have compared them. That they are not the ventral moieties of the true ribs is shown by the fact that they overlap the latter, the two series of structures lying at a different plane in the musculature. I intend to make a more detailed communication to the Zoological Society upon this subject immediately. FRANK E. BEDDARD.

Inheritance of Acquired Characters.

REGARDING the "non-inheritance of acquired characters," the following is interesting:—

I was recently visiting a sugar plantation near Ottawa, Natal, and there was shown four fox terrier pups about a fortnight or three weeks old, two of which had been born with quite short tails, and one with a tail shorter than the normal. The fourth pup had a full-length tail. The mother was an ordinary fox terrier with cut tail. When the circumstance of these dogs being born with short tails was first mentioned to me I refused to believe it; but examination showed that the short tails were really naturally short tails and not tails that had been cut, that is to say, the short tails had at their ends the usual tapering vertebræ of a normal dog's tail, and, of course, at this age it was easy to see that the tails had not been cut or bitten off.

Cape Town, April 7. D. E. HUTCHINS.